that all energy is not equally valuable; thus, in an ordinary gas engine cycle, or that of the Diesel engine, let him assume that when energy has been given up to a crank shaft, and we now expend it in compressing the stuff in a pump, that each unit of it is five or ten times as *valuable* as mere heat energy; it is astonishing how much clearer his notions become concerning real practical efficiency of an engine. He is getting acquainted with the fact that all the problems of the engineer are much more complicated than those of the physicist or mathematician. JOHN PERRY.

VERTEBRATE MORPHOLOGY.

Vergleichende Anatomie der Wirbelthiere, mit Berücksichtigung der Wirbellosen. Von Carl Gegenbaur. Vol. ii. Pp. viii + 696. (Leipzig: Engelmann, 1901.) Price 20*s.*

THE aged master and founder of modern comparative anatomy has completed his life's task, and he has retired from the busy world. The results of half a century's active research and incessant thought are embodied in his "Vergleichende Anatomie der Wirbelthiere," of which the second volume deals with the alimentary and respiratory, vascular and urinogenital organs. The plan of the work now needs no further comment;[1] it is the same as that of the first volume. Short, extremely condensed accounts of invertebrate conditions form a kind of introduction to each chapter, which then deals with the Vertebrata to a very full extent, certainly much more fully than any other general and comprehensive book.

On the whole, the present volume is more up-to-date. Naturally so, since the bulk of it has been written within the last few years, and the author was able to dismiss for ever from his mind the vast amount of matter embodied in the first volume. The whole stupendous work, about 1500 closely printed pages and nearly 1000 text-figures, will give an everlasting impetus to vertebrate morphology. Not only is there deposited in it an enormous amount of anatomical descriptive detail, not only is it full of grandly conceived ideas, but new lines of further inquiry are laid down plainly, with cautioning against lurking pitfalls. It contains all the features of a text-book in the true sense of the word. It is a guide how to study morphology. And still this grand work, although hailed with delight on its first appearance, is not exactly liked or loved. On the contrary, many of its readers are disappointed. Those who expected that the book would be a revelation of the whole of animal morphology would do well to take to heart the author's repeatedly emphasised confession, "dass wir in den meisten Fragen erst am Anfang der Erkenntniss stehen." We, who know little, demand a final solution, where the master himself, after his own dissections and having criticised the conflicting statements of other workers, is satisfied with opening out entirely new vistas which widen the problem. Examples of this are the chapters dealing with the tongue, palate and epiglottis, wherein are embodied some of the author's latest original researches.

But there are several drawbacks which seriously detract from the value of the book as a readily accessible source of information; it is so difficult to understand. Every great writer has his own style, and that of our author is involved and heavy; there are hundreds of sentences the deep and sound sense of which does not reveal itself without much painful interpretation. This is felt and frequently admitted also in Germany. One of his foremost disciples, however, has written an indignant protest against this charge; the apologist points out that the subject-matter itself is difficult, that such a text-book must necessarily stand on another level than a novel, and that Goethe or Kant are likewise not always easy reading, &c.! Well, there are, and will be, many good morphologists and German scholars who will misunderstand our author, and that through no fault of their own. However serious and annoying, this defect of the book is a matter of form.

A much graver consideration is the following. The author begins a chapter with a continuous, needless to say logically coherent exposition of the structure, modifications, the phylo- and ontogenetic development of certain organs, and his own leading view appears clear and convincing until, without warning, he contradicts himself in what he has laid down previously, perhaps in some other chapter. Or worse still, there follow long passages in small print containing another hypothesis or new facts the merits of which are put so forcibly that the reader cannot doubt that this must, after all, be the view preferred by the author. In many of these cases only one interpretation can be right, but the text goes on as if no amendment had been carried. Frequently this upsetting mode of treatment is obviously due to more recent additions or interpolations. Of course, a fair-minded author gives every tenable hypothesis, and if he then states that the solution is not yet final, no more remains to be said. This our author does often, even as a rule, but not always, and, therefore, the exceptions are all the more jarring. *Jurare in verba magistri* may be a sign of unscientific weakness or laziness, but we have a right to learn the views of an acknowledged master.

But let us proceed from generalisations to crucial points.

Origin of Vertebrata.—The transformation of the anterior cephalic portion of the alimentary canal into a respiratory chamber is predominant in, and typical of, the Vertebrata. Resemblances in the formation of such a chamber, first and faintly indicated in Cephalodiscus, carried further in Enteropneusta and Tunicata, do not mean near relationship with the Vertebrata. Direct transitional stages are still unknown, perhaps because the creatures concerned have died out. Mouth and arms being secondary features indicate that the Vertebrata have a long ancestral history. Although Amphioxus resembles Tunicates in many respects (respiratory chamber, peribranchial cavity, hypophysis, &c.), the metamerism of its body is a feature of such importance that it forbids any closer connection of these two groups (p. 25). Thus we are told categorically that we need not look any further in this direction for the vertebrate ancestors. But in spite of this, on p. 338, the epithelial lining of the bloodvessels is spoken of "als ein erst den Tunicaten und von da den Vertebraten gewordener Erwerb." The remark on p. 25—that the origin of the Vertebrata is not quite strange to the Invertebrata, since the organisation of the former exhibits nothing absolutely new, nothing which does not crop up in some one of the other phyla—sounds rather flat, and conveys little comfort to him who is anxious to learn what the greatest authority has to say about their descent.

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[1] See review of vol. i., NATURE, December, 1898.

Lungs and Airbladders, p. 256.—Certain Selachians possess a dorsal blindsac opening into the œsophagus, perhaps the forerunner of the airbladder of other fishes. Since this diverticulum exists in Selachians only during their early life and vanishes in the adult,

> "wird es als rudimentaeres Organ zu deuten sein, wobei nur fraglich bleibt, wie der ausgebildete Zustand sich verhielt, und ob es je einen solchen besessen hat."

Are here not mixed up the two opposites rudimentary and vestigial? If this organ never was in a complete, functional condition, it would be a rudiment in the sense of incipient evolution. But our author can only mean vestige or remnant.

Concerning the question of the homology of airbladders and lungs, a view still frequently advocated, we are told clearly, on p. 216, that

> "from the low stage of the future lungs are derived other organs which do not yet have a respiratory function, namely, the so-called airbladders of the fishes. Consequently we do not meet with the lungs as such from their first beginning, but as air-receiving organs of other significance. Only gradually they become capable of competing with the primary respiratory organs (the gills) and are thereby turned into lungs."

But on p. 256 we are informed, upon the ground of want of proofs of the change of airbladders into lungs, that more likely the airbladders and lungs are akin to each other only in so far as both are evaginations of the gut, but that both have started very early upon opposite roads. In other words, the text is flatly contradicted by the small-printed later addition.

Gills.—On p. 239, the inner gills of the Anura are properly derived from their outer gills, a modification which has been studied by Maurer and others. On the next page, however, the outer gills of the Anura are derived, à la Boas, from the true inner gills of fishes, and on p. 341 we are told that the former, first, derivation means a gap in comparison with fishes. Of course there would be a gap, since the two statements, the first correct, the second a baseless assumption, are absolutely contradictory. This muddle could not have happened unless the small type of pp. 240-241 was a later addition to the text.

Vascular System, pp. 337-339.—The participation of the endoderm in the formation of the heart of Amphioxus is certain. Very valuable is the fact that the endoderm contributes to the vascular system, heart and vessels of certain Elasmobranchs, but it is gradually superseded by the mesoderm. It is doubtful whether the case of entirely mesodermal formation is a reversion to the original condition. Since the endodermal origin stands in opposition to what happens in almost all the bilateral invertebrates, we conclude that the change took place already in the Tunicata, viz. that endodermal growth has been acquired by them, whence the Vertebrata have taken it over. Now, having had to conclude that the endodermal origin of the vascular system of Tunicata and Vertebrata is a secondary feature, which is still preserved in but a few cases, the question arises whether the mesodermal origin is to be explained by a reversion to the original condition or whether it is (p. 339) once more (wiederum) a cænogenetic feature. We desist from answering these questions,

> "da in den Thatsachen nichts geaendert wird, und durch Caenogenese auch etwas Altes entstehen kann, nach den Beziehungen die alt und neu besitzen."

He who understands the above sentence, to the exclusion of doubt, will be able to translate it.

Could the author not apply to the solution of the discrepancy the same principle of the suggestion which he makes on p. 416, apropos of

> "the origin of the lymph-follicles reveals the lymph-cells as derivations of the endoderm. The primitive condition is lost in the Placentalia, not because the endoderm has handed over its function to other tissues, but because the latter have received their endodermal share at a much earlier ontogenetic period"?

Heart, p. 345.—The heart of reptiles, birds and mammals passes temporarily through the stage of a double tube. On p. 345 this is explained as an undoubtedly secondary feature, due to a special adaptation to nutritive arrangements, namely, the accumulation of foodyolk, on or in the wall of the gut. In the Mammalia the double anlage still occurs, in spite of the loss of the yolk. This is certainly an ingenious and possibly correct explanation, but the reader will miss any allusion to Elasmobranchs, with their unpaired heart-tube and great mass of yolk. He may further wonder from which class of animals the mammals have taken over this feature, if, as our author contends repeatedly, the reptiles are to be excluded from the mammalian line of descent.

Coecum and Vermiform Appendix.—On p. 171, speaking of the end-gut, he emphasises his former suggestion that the finger-shaped gland of Selachians is the forerunner of the cœcum; a complex of glands which pour their secretion by one duct into what marks the upper end of the end-gut. Such glands must necessarily have started from the endoderm, but on p. 174 we find the following perplexing statement:—

> "The independence of the cœcum is (in Iguana) most strongly shown, and thereby we come to that organ which the Selachians possess as the finger-shaped gland, originally foreign to the gut-wall, but raised to permanent value by the connection with the latter."

As here expressed, this can only mean that once upon a time there existed a fluid-secreting gland in the neighbourhood of the gut, and which later on, becoming connected with the gut, laid the foundation of an important organ, the cœcum.

Excretory Organs, p. 437.—The author favours Boveri's hypothesis of the evolution of the archinephric duct, but with an almost fundamental modification. However, this explanation, being inapplicable to Cyclostomes, leads to new difficulties and further doubts, which partly undo what has been elaborated just previously.

H. Gadow.

THE WORK OF MARIGNAC.

Œuvres complètes de J. C. Galissard de Marignac. Tome ii., 1860–1887. Pp. 839. (Paris: Masson and Co., n.d.)

It is by no means an easy task to give an intelligible account of the labours of such an industrious and versatile worker as Marignac was. An outline of his life, written by M. Ador, his son-in-law, who edits the two large volumes of his republished work, has already been noticed in these columns. It now remains to deal with his researches, of which only a sketch has previously been given.

The three main lines of investigation treat of (1) the rare metals of their compounds, (2) crystallographic measurements, and (3) thermal chemistry. Under the first heading, we find memoirs on beryllium, lanthanum, didymium, yttrium, erbium, niobium, tantalum, "ilmenium," zirconium, mosandrum, and ytterbium, the last a discovery of his own. The final article in the book, published in 1887, is a criticism of Sir William Crookes's paper "On the Genesis of the Elements"; Marignac is not disposed to accept the interpretation which Crookes places on the different spectra of successive fractions of "yttrium," viz., that a gradual separation of an element into parts endowed with different properties has taken place; he rather inclines to attribute the varying spectra to the accumulation at each end of fractions of impurities, each of which has the power of profoundly influencing the spectrum of the real yttrium.

The equivalents of no fewer than twenty-eight elements were determined by Marignac; and at the end of the book a comparison is made between the values found by him and the table of the International Committee of 1903. The correspondence between the two is very striking; indeed, in no fewer than fourteen instances, the numbers are almost identical. It is strange, however, that Stas found for the atomic weight of iodine the number 126.85, while Marignac agrees more nearly with later determinations by Ladenburg and by Scott. In stating his results, Marignac is always modest. He writes:—

> "Je puis bien reconnaître, après avoir étudié le beau travail de ce savant, qu'il a apporté, dans ses expériences, des soins infiniment plus minutieux que ceux que j'avais cru devoir prendre."

Nevertheless, in almost all cases, the agreement with Stas is a very close one. He is by no means convinced that Prout's hypothesis is put out of court by Stas's researches; he draws attention to the fact that while the mean variation from whole numbers of the atomic weights determined by Stas should be about 0.5, it is only 0.103, even if chlorine be included; and if chlorine be rejected, it is reduced to 0.068.

From time to time, Marignac wrote criticisms of notable papers recently published; and in many instances he repeated the work of the authors. His remarks were always gentle and kindly, hence he never was drawn into controversy. Yet he bore his share in attempting to solve the questions of his day; he published many papers relating to dissociation; the most noticeable deals with the specific heats of gaseous ammonium chloride, mercuric chloride, and sulphuric acid, and the heats of volatilisation of these bodies. The latter are naturally high, for they include the heat of dissociation. Marignac's criticisms are, however, sometimes a little naïve; for example, after drawing attention to Andrews's and Tait's observation that the volume of ozonised oxygen is increased permanently by raising its temperature to 230°, he remarks:—

> "Or, une condensation aussi considérable que celle qui résulterait des expériences de MM. Andrews et Tait eut été un fait trop saillant et trop important pour échapper à ces habiles chimistes (MM. Fremy et Becquerel) ou qu'ils n'en fissent pas mention."

If this species of argument were permitted, the progress of science would be slow.

Marignac's crystallographic measurements are very numerous, and were evidently made with the greatest care; they should form a valuable storehouse of facts, when our knowledge of the relation between the forms of matter and its constitution has been further developed.

Among his researches on thermal chemistry, besides those relating to anomalous vapour-densities, Marignac devoted much time to the investigation of the specific heats, densities, and expansion of solutions. Like all his work, it is careful and exact, but led to no important conclusions.

Enough has been said to give the reader an idea of the enormous productivity of Marignac. In his own field, that of the rare earths, he is probably unsurpassed as an investigator, and in issuing this collection of his memoirs, M. Ador has erected to him a monument "aere perennius."

W. R.

IRRIGATION IN THE WESTERN STATES OF AMERICA.

Irrigation Institutions. A Discussion of the Economic and Legal Questions created by the Growth of Irrigated Agriculture in the West. By Elwood Mead, C.E. Pp. xi+392. (New York: The Macmillan Company; London: Macmillan and Co., Ltd., 1903.) Price 5*s*. net.

This work was originally prepared for a course of lectures on the institutions and practice of irrigation for the University of California. The author is of opinion that the land in the United States that has hitherto been relied on to meet the demands of the nation's growth will not much longer be available for this purpose, so rapid has been the increase